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THE PHYSICAL BASIS OF CONDUCT.

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My purpose in this paper is to consider the question as to what is the character of those physiological processes and structures, which, found within the living organism, form either directly or indirectly the basis of conduct, or, stated more generally, it is to determine the relation which these organic things and events bear to inorganic. Now I am quite aware that there are positions which at least seem to differ very much from the one herein developed, and that which I find to be their common characteristic is the insistence on the point that there is a very considerable group of organic phenomena, found, for example, in the processes of secretion and absorption, of development and regeneration, that can neither be 'reduced to' nor 'built up from,' at least step by step, the physical and chemical phenomena of which we have such an exact knowledge elsewhere. Yet it is clear, that, before such a position or any conclusions based upon it can be admitted, it is necessary not only to determine just what is meant by 'reduction' and by 'building up,' but also to consider if it is not possible, upon the experimental basis on which we now stand, and by means of certain methodological principles, to say just how different the organic is from the inorganic realm, and whether it is determined teleologically, etc. It is this program, then, that I purpose to carry out in this paper.

Accordingly, although at the risk of offering the trite, but yet because it is necessitated by the character of my argument, I begin with the consideration of some points concerning those laws which are uniformly accepted as valid for inorganic phenomena.

These laws, I find, can be divided into two classes, namely, those, on the one hand, which are *general* or *fundamental* in that their

'essential characteristics' are just those which are common to, on the other hand, a group of *more specific* laws, which can accordingly be called *empirical*. Concerning both kinds of laws four questions can be asked: (1) What are they? (2) What is their relation to each other and to concrete phenomena? (3) Are all, or only some of them valid for the organic realm? and (4) Are there, *besides* these, also special organic laws?

To answer question (1). First, there are *four general and fundamental laws* which, as stated in one form or another, are accepted for all physical or at least all inorganic events. That they are also valid for all organic events and qualities I purpose showing. I shall present here only those details which are required for the construction of my argument.

There is the First Law, that of the *conservative* of energy, which may be stated: In an isolated system in which events are occurring the energy-quantum remains constant.

A *system* consists of a number of energy-quanta of different forms coexisting within certain spatial limits; this may be regarded as a *mechanism*, which is not the same as saying that it is explainable by, or reducible to, *mechanical* principles.

The *process* taking place in a system will consist in the change of a definite quantum of one energy-form into that of one or more others. This constitutes *transformation*, the conditions for which are stated only by the generalized second law, but which itself here leads to the second statement of conservation, namely, that

In all energy-transformations the quantum of one form disappearing is equal quantitatively to the total quantum of the other forms which appear.

This gives the *first constituent of determinism*, the demonstration of which is the first point in my argument; it demands that there can not more happen than there is sufficient cause for; there is a *singularity* of effect.

The criterion of that which shall be regarded as a new energy-form is that it is such a quantity as will produce the same effect quantitatively as that which is already accepted as energy, namely, heat and motion. Accordingly, let the claim for a vital energy be advanced; then this must stand the test of this equivalence-transformation criterion, so that, with positive results, such a new energy would be subject to the same general principles of 'mechanism' as are the other energy-forms.

The Second Law states the *condition for transformation*. According to it, in its generalized form, each energy-form is the product of

two factors, an extensity or capacity and an *intensity* or *potential*, these of course, being different in different species. Secondly, and most important, it is in virtue of intensities that events take place; here there is *another deterministic element, the second:* Two intensities may be opposed and meet, as it were, at a common point; they must, too, be either equal or unequal. With the latter the case, *i. e.* with a difference of intensities existing, and with no third intensity present to supplement the lesser one, *i. e.* to *compensate* the difference, an energy-transfer, *an event*, must take place in the *direction* of higher-to-lower-potential until equilibrium is reached. Thus the principle of efficient causation receives exact formulation, constituting the 'law of events.'

The Third Law, entropy. Since most, if not all events, are exothermic, since, too, because temperature (heat intensity) can not be compensated, heat spreads out or is dispersed, and since, as it does this, the condition of a uniformly equal temperature is approached to, there results a *third constituent of determinism;* natural events have an *irreversibility*, a *definiteness of direction;* the entropy of the universe increases.

It is important, also, that this law, in some of its features, is derived from the Second.

That which may be regarded as the Fourth Law, determinism, is, as has been indicated, derivable from the three preceding: Conservation gives singularity, uniqueness of determination; by a cause one and only one effect, namely, that which is equal to it quantitatively, can be brought about. Likewise the Second Law; according to it there is only one effect, namely, that which, as the total rise of one or more intensities, is quantitatively equal to the fall in that intensity which is cause. And it has been seen that the Second and the Third Laws mean a definiteness of direction, an irreversibility in events. These and singularity together constitute *invariability* or *determinism* in the events taking place in any system or mechanism.

The First and Second Laws, and, accordingly, the Third and Fourth by virtue of their derivation from them, can all be expressed in their generic form by an epitomizing formula[1]

[1] The derivation of this formula I have shown in my paper, The Energy of Segmentation, *Jour. of Exper. Zoölogy*, June, 1907. In it, W signifies the external work produced by any reversible change taking place in any system at a constant intensity, I; $U_2 - U_1$ is the accompanying change in the internal energy of the system; dW is the increase in the quantity of work produced when the same change in the system takes place at the intensity $I + dI$; dW/dI is the potential coefficient, I the intensity; the resultant change in the energy of the system is the product of the two.

$$W + (U_2 - U_1) = I\frac{dW}{dI},$$

which can be demonstrated to stand in the relation of genus to a certain class of empirical laws, namely, specific energy-laws, as species. This fact is of great importance for our subsequent considerations.

According to the above principles then, *every event in the physical world is really identical with an energy-transfer under the conditions stated;* and each energy-form is the product of an intensity and an extensity, *i. e.*,

$$E = ic.$$

However, since usually only the changes in, and not the absolute quantity of, the energy of a system can be measured, the differential form of this equation,

$$dE = d(ic) = idc + cdi$$

is more generally applicable.

Then $dE = idc$ when i is constant, and cdi with c constant.

Attention is drawn to this point to make clear what the ultimate purpose of empirical investigation *may be.* In general this can be said to be that of determining dE in any way possible, but as the change, always, of some specific energy-form.

In some cases, now, this end can be attained *directly*; the energy-change can itself be measured. With this done, and with it at the same time also possible to measure the absolute value of one of the factors, it is evident that, should this also be desired, the value of the change in the other factor can be computed; or, conversely, with the *change* in one known, the absolute value of the other factor can be found. In other cases, however, the energy-change can be determined only *indirectly* by measuring both the absolute value of one factor and the change in the other.

Indeed the fact of these possibilities makes it evident that the purpose of empirical investigation may be twofold, namely, to get directly or indirectly, as the case may be, *laws*, which will express either (1) the *transformation* of an energy-quantum of one kind into that of another, *or* (2) the *functional relation* (*a*) between the two factors of the same energy form, or (*b*) between the intensity factors of different forms, or, finally, (*c*) of constituents of such factors.[1]

[1] Examples: (1) $U_2 - U_1 = Q - W$; (2) (*a*) $p_1v_1 = p_2v_2$; (*b*) $dp/p = dT/T$; (*c*) where, for surface energy, $dE_s = \gamma ds$, and $\gamma = \frac{1}{2}grhD$, these last are constituents.

Whatever now the term may be that would best characterize and distinguish these last three from the distinctly energy-laws, I for that purpose shall call them functional, descriptive or empirical; and furthermore I wish to insist on that which is perhaps quite clear, but yet important, namely, that they can usually be made constituents of energy-laws, and accordingly must in this sense be in thorough accordance with these.

These facts have been presented in order to make evident what is the character of the relation which the Four Laws bear to empirical laws and to concrete phenomena. However, with regard to this, two positions, *each conditioning a typical and a distinctive view as to the nature of organic* phenomena, can be found and must here be stated.

One which I designate α is, that, while admitting the Four Laws to be at least predominantly quantitative, they nevertheless express the common characteristics of the series of specific energy-laws and thus ultimately also of concrete phenomena — for these are energy-changes — so that there results a natural classification, with the Four Laws, or the equation epitomizing them, as highest genus, with concrete events as *infima species*, and with specific energy-laws as intermediate concepts. Accordingly, the characteristics formulated by the Four Laws extend, in all their aspects, both logical and alogical, down and through the empirical laws and become *incorporate* in the concrete phenomena so that these are simultaneously and cospatially both quantitative and qualitative. It is this view which is indicated, I believe, by practically all the considerations which bear on the question, to be correct one, and this it is accordingly that I purpose to support.

The second position, β, diametrically opposed to this first, is that the Four Laws *are not thus incorporate;* they are interpreted as not touching, or, better, as not affecting the qualitative side, but rather as expressing *only* the quantitative aspect; this, therefore, is regarded as existing simply *side by side* with the qualitative, but not as 'penetrating it.'

However, the reason why this position is held becomes evident by considering the use which is made of it, and is shown clearly to be an ulterior one. In those who maintain it the *conviction* is strong that organic phenomena are very fundamentally different from inorganic, that accordingly they have their own distinctive qualitative laws and may be characterized by the presence of a teleological element. The opportunity is then sought, while holding this position, and interpreting the teleology as contradictory to determinism *formally*, to avoid at the same time any contradiction with it *realiter;* and it is by making

this second interpretation that this opportunity is believed to be found. For, with it a fact that some, perhaps even many, of the phenomena in the organism are different from those in the inorganic realm, it is argued, that, if this qualitative side be not affected by the Four Laws with their determinism, there will be found room in it for the operation, not only of distinctive, qualitative organic laws, but also, without *concrete* contradiction with determinism, of a teleology.

Yet, that this attempt is really successful, is, I think, open to much doubt; but it has the value of indicating rather clearly the point around which our problem turns, namely, *the relation which one law may have to another*. It is to the consideration of this, then, that I now proceed. Of such relations I find that there seem to be three possible cases:

(*a*) There is first suggested, as an exaggerated analogy of the above second position 'β,' the case of two laws applying to concrete phenomena which are widely separated in time and space; then these laws might of course be different, very different; in fact it might appear and even be claimed that they could be so different as to have reached that degree of difference which, as stated *formally*, is *contradiction*, and yet that this, in virtue of the remoteness of the things to which they apply, would not carry with it a contradiction *realiter*. However, such a remoteness could, I think, not long be maintained; sooner or later, either through their implications or because ground for their application *within the same complex* might apparently be found, the two laws would be brought into 'contact' at or 'penetration' of that which can theoretically be regarded as the same point. That point, then, would, by assumption, be determined simultaneously in two *divergent* or *opposed* directions, *i. e.*, a *contradiction realiter* would be generated. The supposition then that the two laws could be so different as to be contradictory is shown to be impossible; then we must infer either that they must be contraries and as such have certain characteristics in common so that they will be subordinate to the same general principles, or that they must have the relation to each other of genus and species. In the first case their determinations would be regarded as running *parallel* and in the same direction, and not as opposed or divergent; in the second, they would coincide, *i. e.*, those of the genus would be incorporate in those of the species. The former leads us to

(*b*) Two laws, coördinate with each other, may be valid within the same system, and apply to different processes. Their determinations of these processes will then run parallel and in the same direc-

tion, while that of the system as a whole will be in accordance with those more general principles to which the specific laws are subordinate. The two laws, then, will be valid side by side, parallel to each other; they will be different, but, again, not contradictory.

(*c*) But, given what are supposed to be two laws or principles, and grant that they actually or seemingly apply to *one and the same process*. What can their relations be? *Three* cases can be distinguished: (1) The two might prove to be contradictory; *i. e.*, the attempt to apply them to the same process might disclose that they respectively assert and deny the same property or character in that process; for it is just this condition of 'penetrating' the same point or process that generates a contradiction *realiter*. Then both can not be true; and the attempted application of one will have failed; or (2) the two might be laws or expressions for incorporate aspects of one and the same process; as such they might prove to be constituents of one and the same law holding for the process as a whole. Then they are perfectly compatible, in no case contradictory; or (3) between the two laws there might be the relation of genus and species, of subordination one to the other; then the determinations of the one would not run parallel to those of the other, but, rather, the genus would be incorporate in the species and both in turn incorporate in the concrete process. Although different, as stated formally, the two are, of course, quite compatible.

The purpose of this analysis has been to prepare the way for the consideration of the question: If a teleological principle were accepted as operating in organisms, under which one of the above cases is the relation which it bears to mechanistic determinism to be placed? However, it is very evident that the answer to this would depend on the interpretation or definition given to these principles, just as conversely, these would depend on the place in the scheme in which they are put. But we may try the placing of them in each of the different cases and draw our conclusions.

Thus, first, suppose determinism and teleology to be two distinct and approximately coördinate principles, corresponding to simply two different laws as in case (*a*) or, since (*a*) leads to (*b*) suppose, preferably, as in (*b*), that the two apply to different processes in the same system. Then this supposition demands that view of the relation of the Four Laws to concrete phenomena which was called 'β', *i. e.*, that the mechanistic determinism which these laws imply hold only for a quantitative side or process, the teleology for a qualitative. But our

scheme then demands that the two should not be contradictory; that they should have some generic features, that their determinations should be parallel and in the same direction. Accordingly, the teleology could not be interpreted in its usual way, namely, as meaning that variation of means to an end which is contradictory to determinism, so that if the term be still retained, its meaning must be modified.

However, this supposition that mechanism and teleology are two distinct and possibly coördinate principles must be given up, if, instead of accepting position 'β,' the 'first' interpretation, 'α,' that the determinism is incorporate in the qualities, be postulated. For then the *determinism must apply to the very same process as does the teleology*, and accordingly we get the following possibilities as given by case (c):

Either, (c) (1), the determinism and the teleology are contradictory *realiter; i. e.*, that formal contradiction which appears from interpreting the teleology as asserting a variation of means to an end, and the determinism as denying this, becomes one *realiter* when it is necessary to apply both to the same process. Then both can not be true, and the establishment of one is invalidation of the other; accordingly, either this must be given up, or, if the term be still used, this can be done only with a modified meaning. Thus, (c) (2), if the determinism shall have been proved for *all* processes within the system and so for the system as a whole in all its aspects, qualitative as well as quantitative, and if it still be insisted that there is a teleological determination for the system, then this can be interpreted as meaning only some such thing as *accumulation* or *progression*, etc., but not as variation of means, and the two principles would express incorporate characteristics of one and the same process or system and might be either constituents of the same law or one be a species to the other as genus. However, this last relation would come under (c) (3) according to which determinism and teleology must be not only quite compatible, but one, say, the latter, must prove to be a special case of the other; thus teleology might, as species, be quite deterministic and yet be genus to a certain group of distinctly empirical yet completely causal organic laws.

Can, now, a thorough-goingly deterministic position, one which will, without exception, apply to *all* phenomena within the organism, be established? I believe that it can, and, accordingly, to do this is now my purpose; for, with this accomplished, our scheme will enable us to decide as to both the existence and character either of a teleology or possibly of other specific organic laws.

The two opposed positions concerning the relation which the Four Laws may bear to the qualities have already been presented. I shall begin my argument for determinism at this point, then, with some considerations as to the *qualities* themselves. That term I use here in perhaps rather a broad sense, equating it with *character* or *property*; but the meaning which I attach to it may be indicated by the statement that it is by the qualities or properties that the bodies and substances which we perceive and know are made up or constituted. For there concern us here, first, the three categories of existence, things, events, and relations, and then, fourthly, that by virtue of or in respect to which any of these may differ or be similar among themselves, namely, qualities.

But 'things' seem to have a precarious existence; they change, either slower or faster. But changes are energy-transfers; so I will *let thing = system*, as I have previously defined it.

Now a system, or 'whole,' implies 'parts,' but as to what is regarded as '*whole*' and as '*part*' differs not only with different sciences, but also, within any one science, both with the period in its development and with the immediate purpose in view.

However, whatever be the system which, for the time being, is regarded *as the* '*whole*,' and however its qualities and properties be perceived, there is one principle which I find to be operating in all cases of 'whole and parts,' the principle, namely, which I call "creative synthesis." Its presence and 'working' can, I think, be demonstrated as follows:

Whatever may be the qualities which a system has as a 'whole,' *i. e.*, whether they be processes, or qualities in the narrow sense of the term, there is always a division which can be made among them. For, on the one hand, there are some of them which are the same as those characters which the 'parts' or 'elements,' whatever for the time being may be selected as such would have or retain if isolated; these accordingly give an *additive* result in the 'whole.' On the other hand there are other properties which are not to be so derived (additively), and it is here that the important principle of '*creative synthesis*' appears. All such *non-additive* characters can and must result only from the coöperation of 'parts' or 'elements' which, when isolated, have characters different from those that are evident when a 'whole' is formed by their coëxistence. The 'parts' *determine*, *cause*, the appearance of the qualities, and the qualities are new; in some sense they now exist where before they did not. This principle is operating when electrons coöperate to form an atom, atoms molecules, and mole-

cules a particle, etc.; indeed it can be shown to be quite as valid if it be insisted that there are 'secondary' qualities as opposed to so-called 'primary'; to do this it is necessary only to bring the perceiving subject into the 'system.'

Now, it is the recognition of this principle that makes it possible to give a definite statement as to what constitutes 'reduction,' and thus both to answer one of the questions raised at the beginning, and to make the next step in the demonstration of determinism:

Given that which is for the purpose in hand selected as the 'whole,' and letting its 'parts' also be known, say either as atoms, or as ions, or as molecules, or as constituent energies, then the qualities and properties of each of these may be enumerated and laws, of some kind, — what, we shall see, — may be given. For there are three cases:

1. In some instances it will be found that the qualities of the 'whole,' let this be either a body or a substance, result *additively* from those of the 'parts';[1] the former may then be said to be 'reduced' to the latter. This additive method constitutes 'reduction' of the *first kind*. Concerning it two statements must be made; first, if *all* qualities were additive results, then all the physical sciences could adopt a simple computative, perhaps purely deductive method, and secondly, with determinism valid for the 'parts' it would also be for the 'wholes,' and conversely.

However, in a great many cases additive derivation is not possible; and the evidence shows that this is *due, not to our ignorance*, but to the peculiar way in which the 'parts' work together. It is here then that 'creative synthesis' is operative.

Again, such cases fall into two classes. Common to both of them are the facts, first, that, some body or substance having been selected as the 'whole' or complex to be investigated, descriptive laws, coëfficients, etc., of the properties of this as a 'whole' can be found and second, that it will be known that such a 'whole' is made up of certain 'parts,' *e. g.*, ions, atoms, specific energy-forms, etc., the laws of

[1] Examples of such additive results could be given almost *ad infinitum;* I content myself with giving some typical ones from the strictly scientific field; thus, 'specific refractive power,' 'specific absorptive power,' 'specific rotary power' of a salt-solution are the additive result of those of the ions; likewise 'specific volumes,' 'molecular heats,' and the pressures of gases or of substances in solution are additive. In all such cases an empirical law, descriptive of some property of the 'whole', can be found, independent of any similar law for the part; yet when this latter is also obtained, the two are found to be related additively.

whose properties and behavior are also known, but yet that such properties are not the same as those of the 'whole.'

2. But at this point the division begins; for, with so much known, it is possible in *some* cases to discover, although only empirically, an exact functional relation, constant within certain limits, between certain laws of properties of the 'whole,' and those of the 'parts.' Such a 'connecting law' is really a law of the synthesis; its importance here is that it gives a second meaning to the term 'reduction.'[1]

3. ~~(3)~~ In other cases *no such functional relation* is discernible; at present we must content ourselves with laws of the 'whole,' and of the 'part,' but without connecting them empirically. 'Reduction,' even of the second type, is not possible.[2] Shall there be inferred, then, from this lack of 'reduction,' either a permanent irreducibility, or an indeterminism of the qualities of the whole? The question is especially pertinent, since most of our present knowledge of the organism, as found in physiology, histology, and embryology, etc., is of this third type.

For, on the one hand, it must be granted that there are present in the organism as a 'whole' certain qualities and processes which are found nowhere else; it must be admitted, then, that these distinguish the organic realm from the inorganic and may be regarded, in some of their phases at least, as specifically organic characters. But, on the other hand, that just this should be the case, our principle of 'creative synthesis' makes quite intelligible, quite probable. For the organism is a complex, a 'whole,' made up of 'parts' — let them be the atoms or certain ions or the colloidal particles, — which, taken singly, *are found* elsewhere, but which *taken together* are found only in this

[1] Examples of this are: (1) Kinetic theory of gases, according to which the temperature of the whole is functionally connected with the motion of the parts; here *the fact is* that temperature and motion are not the same thing. (2) The *color* of bodies can *likewise* be closely connected with the absorption of light and this in turn with certain atomic phenomena. (3) Increased pressure of 'dilute solutions' as a function of *number* of ions. (4) Optical activity (rotation of planes of polarized light) as a function of spatial position of atoms in the molecule. Finally, I think, all so-called 'reduction' of other qualities and properties to those of the attraction and repulsion and resultant motion of mass particles is of this kind.

[2] Examples of this class could also be given in almost indefinite number, but a few chosen at random suffice: — crystal-melting-points, coefficients of elasticity, of surface tension, of expansion of metals, condensation point, etc. These can be found both for compounds and for constituents. Now, we are not able to make use of the 'conceptual model' constructed by the aid of elementary corpuscles with ideal motions. Much of the behavior of the lower organisms, as described by Jennings, for example, falls in this class.

special complex. And since the qualities of such a 'whole' result, some few of them additively, others by 'creative synthesis' from those of the 'parts,' then, so far as the last is the case, the qualities of this complex must be those which will be found in no other complex (inorganic). And so far as these qualities or some of them can not *now* be 'reduced,' in either sense of the term, all that can be done, though even this forms the first step in 'reduction,' is to study the qualities of the 'whole' directly and by themselves, and, if possible, get empirical, descriptive laws for them. In this respect, the position — sometimes taken — that the organism itself is the only useful unit for biological study is justified.

But this admission does not warrant or allow an evasion of the question as to whether physical laws, all or only some of them, are valid for the organism. To this the first answer that can be made is quite evident from the principle of 'creative synthesis': Those physical laws which are descriptive laws of inorganic 'wholes' or complexes are limited to these unless such 'wholes' are themselves found in organisms and give additive results; however, in general, such laws are no more valid for 'organic wholes' than, conversely, the peculiar laws of the latter are for the former. And, secondly, so far as certain parts, certain chemical elements, etc., are not among the constituents of the organism, it is evident that their specific laws have no application to it.

But, on the other hand, it is a matter of well authenticated knowledge that the organism, or its unit, the cell, is made up of the atoms of certain elements, these atoms constituting an energy-form of a very specific character; and the presence of other specific energy-forms is also established. Furthermore, these constituents, when isolated, are known to follow definite specific laws, either themselves 'energy-laws,' or constituents of these. But these have been demonstrated to be subsumable under the Four Laws, so that, conversely, the determinism which they imply is incorporate both in the specific laws and, finally, in the concrete phenomena to which they apply; for concrete phenomena, at the same time that they are qualities, are also quantities. The qualities are, therefore, completely *determined*.

Accordingly, it must be granted, in agreement with general methodological principles as to the 'extension' of a law and as a matter of consistent procedure, that, when the atoms of certain elements and a number of specific energy-forms coexist in a certain complex, all the laws for these, generic (like the four) as well as specific, are still obeyed. Indeed, there would be no question as to this were all the qualities of such a complex or 'whole' derivable additively; then, the

laws of the 'whole' would be derivable in like manner from those of the 'parts.' But no more should there be a question as to this validity when it is found that this additive derivation is limited, and that 'creative synthesis' must be brought in that an account may be given of *the appearance of new and distinctive qualities*. For, whether these can be 'reduced,' in the second sense of the term, to the properties of the parts, or not, in neither case can any reason at all be given why the fact of this 'synthesis' should invalidate the continuance of the 'concrete operation' of either the special or the general laws. In fact, for inorganic complexes it is accepted that there is no such invalidation. Then, I hold, — and this is perhaps one of the most essential points in my argument — *consistency* demands that the same position should be taken for organic complexes; *here* there are quite the same reasons — no more, but certainly no fewer — for it as *there;* the only difference is one of constituents and the arrangement, etc., of these within the complex. Consistency demands, then, that, in the view which we take of the results of the investigation of organic phenomena, we should adopt simply the same principles of procedure and of interpretation as are accepted for inorganic. For then, and then only, can the character of the relation between the two realms be equitably stated.

This brings the result, in both cases, that, just as the relation of the Four Laws to the specific laws and to concrete phenomena implies a determinism for them when taken singly, or together additively, so will that same determinism be quite as valid for them and the distinctive and new qualities which they produce when working 'synthetically.' For these qualities, as qualities of the 'whole,' there may be discover able — as I have pointed out — laws, descriptive and even specifically organic.

But these laws, by virtue of the origin of that of which they are laws, will be subsumable under the same principles of mechanistic determinism as are the qualities which, though *new*, are yet, like those of the 'parts', quantitative as well as qualitative.

The meaning of all this is, of course, a complete and thoroughgoing determinism for all processes and qualities found within the organism, *i. e.*, for those both of the 'parts' and of the 'whole', and for the 'synthetic creation' of certain of the latter from the former, whether the functional relation between the two be known or not, and it was this position that I set out to establish.

Considering this attempt to have been successful, further results may now be stated as they are demanded by the scheme of the 'possible relations between two laws or principles.'

That analysis shows (*c*) (1) that, with determinism established for the organism both in 'whole and part', there is absolutely no opportunity for teleology, if this mean 'the variation of means to one end'; for, with the determinism holding good for all qualities and processes, the attempt to apply teleology in the above sense to any one of these makes of the formal contradiction one *realiter*. Consequently, if it still be insisted that the term 'teleology' be retained, its meaning must be modified, (*c*) (2). And there are, indeed, certain good reasons for so modifying and retaining it; for, on the one hand, the principle of 'creative synthesis' makes it intelligible that the organism, since it is a 'peculiar' complex, should have certain properties and processes not found in the inorganic realm, and yet that all of these should be completely determined. However, among these there might well be some, which, as indeed there are good grounds for maintaining, could best be described *empirically* as *directly preservative of both the form and life of the organism*. On the other hand, the same principle makes it easy to understand, that in both the ontogenetic and phylogenetic development there should be a *progression* in the appearance of new characters, and, in this sense, as is the case with every *progression*, an *accumulation*. The progression, at the same time that it would be quite determined, would also imply at each stage both some end previously 'made for' and the making for a more distant end in the same series.

At length, then, it is possible to answer one of the principal questions stated at the beginning of this paper, namely, as to the nature of organic events and qualities and their relation to inorganic. For the determinism which I believe to have found means that the Four Laws extend down and through — as the genus does for the species and their constituent characters — the empirical laws, both energy- and functional and descriptive, of the organic as well as of the inorganic realm, and that they finally become *incorporate* in the concrete phenomena. But the development of the position has shown that this leaves ample opportunity for the admission of the claim made by the adherents of *another*, a contrasting, viewpoint, that *some* organic phenomena can not be 'reduced to' or 'built up from' inorganic elements or 'parts,' or the laws of the former be connected functionally with those of the latter. What such a 'reduction' or 'building up' must mean has been analyzed, and the unjustifiableness of inferring the permanent impossibility of 'reduction' of the second type from its present lack has been made evident. On the other hand, that there must be perhaps such a permanent impossibility as concerns additive reduction, the principle of 'creative

synthesis' has made quite clear. At the present time, then, *some* organic phenomena are 'reduced' according to the first method, *others* according to the second, while for still others neither of these is possible, so that, for these last, the only recourse is the descriptive law, and the most expedient unit perhaps the organism, or the cell, or the nucleus, and not the atom, the energy-form, or the colloidal particle. But, at almost any time, it may be found that such descriptive laws are functionally connected with those of the atom, or of the specific energy-form. Accordingly, they *must be held to be* quite compatible with, in fact, theoretically, to be *cases of determinism*, at the same time, that, as descriptive, they may be best stated and worked out from the standpoint of the teleology as defined in (*c*) (2), that, namely which is conducive to the *preservation* and *progress* either of the individual or of the species. *Such a teleology* is (*c*) (3), not only compatible with, but *a special case of determinism* itself; it is *objective* and not subjective, *i. e.*, it demands no psychically felt purpose. In fact, the same argument by which determinism has been established, and teleology limited and made acceptable, demands, that, when there are certain events, physical or psychical, identified with *conduct*, they shall conform to these deterministic principles. The psychical teleology becomes, then, a special case of the objective.

Finally, the validity of the Four Laws for organic as well as for inorganic phenomena brings both these realms into the *same classification*, at the same time that in this the presence of *non-additive differentia* as qualities of the 'whole' is both demanded and made intelligible by 'creative synthesis.' Both the organic and the inorganic are, then, species in the same 'natural classification,' that is, the complexes or 'wholes' of the former differ from those of the inorganic realm just as the complexes within this realm differ among themselves. This, then, is the character of the relation which the organic has to the inorganic; they are related as species under the same genus.

PSYCHOLOGICAL LITERATURE.

COMPARATIVE PSYCHOLOGY.

The past year has been one of great activity so far as concerns the work upon lower organisms. Many separate research articles, and one or two important books have appeared. The present resumé cannot hope to do more than give a brief notice of the researches coming under the reviewer's eyes. Attention will be given mainly to American researches. Notice is called to the yearly reviews of Bohn in *L'Année Psychologique*. (Also see Yerkes' very excellent summary of Bohn's own experimental work. *Jr. Comp. Neurol. and Psychol.*, XVI., 1906, 231–239.)

The science of behavior is now so thoroughly established that the present reviewer feels that the time is ripe for setting aside a journal devoted exclusively to its needs. Articles on behavior now appear in the PSYCHOLOGICAL REVIEW, in the *American Journal of Psychology*, in the *Journal of Comparative Neurology and Psychology*, in the *American Journal of Physiology*, etc. Some even are published privately. With a journal devoted exclusively to behavior, all researches should be consolidated, and such a consolidation is to be devoutly hoped for. It is becoming more and more difficult to give the oncoming student immediate orientation in the contemporary research work. Such a journal, supplied with a monograph supplement series would undoubtedly be successful from a financial point of view, if four or five of the leading universities would agree to subsidize it and to publish all research on animal behavior therein.

THE BEHAVIOR OF LOWER ORGANISMS.

Behavior of the Lower Organisms. H. S. JENNINGS. The Macmillan Co., 1906. Pp. xiv + 366.

Jennings has consolidated his own many experimental and theoretical papers in book form. The volume under review represents the printed form of his lectures as given at Columbia University. The book as a whole is very much better written as regards clearness and style than the Carnegie volume previously reviewed in this BULLETIN.

The book is concerned mainly with a discussion of the behavior of lower organisms as determined under conditions of control. At the

end of the experimental division of the book, he attempts an analysis of the factors entering into the behavior of lower organisms. He emphasizes, as in his previous work, the importance of internal factors (changes in physiological state of organism) in the determination of causes leading to new adjustments. He raises the general question as to whether the behavior of lower organisms can be completely expressed in terms of reflex and automatic acts, answering the question in the negative. The reviewer does not wish to be considered as taking a position hostile to Jennings, yet it seems to him that the author in this work is taking a very narrow and mechanical view of human reflexes and automatic activities, and the extent to which they are dependent upon and are modified by the changing physiological state of the organism as a whole.

Unfortunately, Jennings, while not a psychologist, has nevertheless in this volume wandered off into the green pastures of the psychologist (and has even nibbled at the stubble of the philosopher)! He was not content to allow his experimental facts to stand as facts, but must needs raise the question which stands (needlessly) as the bête noir of the student of behavior. Are the lower organisms conscious? Or, to phrase it from the objective standpoint, "Do there exist in the lower organisms objective phenomena of a character similar to those which we find in the behavior of man?" Jennings gives an affirmative answer: "So far as the objective evidence goes, there is no difference in kind, but a *complete continuity between the behavior of lower and of higher organisms*" (italics ours). Jennings then goes on to say that "no statement concerning consciousness in animals is open to verification or refutation by observation and experiment." . . . "All that experiment and observation can do is to show us whether the behavior of lower organisms is objectively similar to the behavior that in man is accompanied by consciousness. If this question is answered in the affirmative, as the facts seem to require . . . then it may perhaps be said that objective investigation is as favorable to the view of the general distribution of consciousness as it could well be." It is at this point that we must raise the question which is fundamental to our science. Have we any other criterion than that of behavior for assuming that our neighbor is conscious? And do we not determine this by the complexity of his reactions (including language under behavior)? Complexity in conscious content is always accompanied by complexity in adjustment. This idea is the basal one in functional psychology. If my monkey's adjustments were as complex as those of my human subjects in the laboratory, I would have the same

reason for drawing the conclusion as regards a like complexity in the mental processes of the two. Nor would my opinion 'then be largely dominated by general philosophical considerations drawn from other fields.' My inferences would alike in the two cases be based upon observed *facts* of behavior. Jennings has not shown, nor has any one else shown that the behavior of lower organisms is objectively similar to that in man. To make the reviewer's position clear, Jennings' statements concerning the presence of perception in lower organisms may be cited. "When we say an animal *perceives* something, or shows a perception of something, we base this statement upon the observation that it reacts in some way to this thing. On the same basis, we could make the statement that amœba perceives all classes of stimuli which we ourselves perceive, save sound (which is, however, essentially one form of mechanical stimulation). Perception as judged from our subjective experiences means much more; how much of this may be present in animals outside of ourselves we cannot know." The flaws in Jennings' psychology are surely patent to every student of experimental psychology. Is simple reaction to a stimulus the only *objective manifestation* of perceptual behavior in man? Certainly not! There are hundreds of others beside the overt movement of the voluntary muscles which can be directly observed, such as eye movement, convergence, accommodation, changes in respiration, circulation, changes in tonus of musculature, etc., and still others which can be inferred, as concerted reaction between different cortical systems; cortical 'retention' of the modifications of past stimuli, etc. It is the task of the experimental psychologists to refine upon and to add to this list of objective manifestations of the perceptual act. So far as we know, some such complexity in adjustment is *necessary to every perceptual act.* If we may be allowed to call introspection in at this point, we find that it everywhere supports our contention that where you have complexity in content you likewise have complexity in adjustment; if subjectively to the human 'experiencer' there is more than simple reaction towards a stimulus in a perception, objectively there is more there too. If Jennings would show that the adjustments of the amœba to a sensory stimulation were as complex *from the objective or behavior standpoint* as our own adjustments to a like stimulus, we would not only be willing to grant him that his amœba *perceives* but also we would be forced to make the assumption for the very same cogent reasons that we assume that our fellow man perceives.

The same lack of psychological analysis is to be found in Jen-

nings' assertions that lower organisms behave as though they consciously discriminate, and that they react as though they had representations.

From the standpoint of the contribution of facts, the book is exceedingly valuable. That portion of the book dealing with the analysis of behavior has a somewhat doutful value because of its vagueness and complexity, and its constant allusions to pleasure and pain and to other psychical processes in man. The final chapters dealing overtly with the relation of the behavior of lower organisms to psychic behavior should be undoubtedly greatly modified when the book comes to a second edition. J. B. W.

The Dynamics of Living Matter. JACQUES LOEB. The MacMillan Co., 1906. Pp. xi+ 233.

It is with a good deal of hesitation that we review this book. When a psychologist attempts to read the writings of Loeb, he is apt to become thygmotactic and stereotropic, and lose that calm intellectual poise which he ordinarily possesses. But inasmuch as Loeb in all his papers is constantly attacking the psychologist, whom he calls the metaphysician (he has not yet learned to make the distinction between modern psychology and modern metaphysics),[1] and since his writings are read widely by biologists, it behooves us for the sake of the young biologist to say a word in our own defense. It is all the more imperative to do this now, because the relations between the students of comparative psychology, and those of biology are becoming closer and closer. And if peace is to be maintained, there must be a certain breadth of mind on both sides, and a certain mutual insight into the principles of each other's science. What is still more important, and this is what Loeb especially ignores, each side must recognize that the fundamental metaphysical assumptions of the other are different; matter, force, energy, etc., on the one hand, mental processes, simple and complex on the other. In Loeb's book, he gives us to understand that he will have given an ultimate explanation of life when he shall have ruled out all metaphysical explanations and substituted 'physico-chemical' ones in their place.

A few sentences which certainly 'do him justice' and which lose nothing of their clearness and cogency by being 'separated from their context,' quoted from the book under review, may serve to show Loeb's wilful misunderstanding of psychology. "What do we know concerning the nature of these automatic mechanisms? *Metaphysics*

[1] He is not even aware that there is a legitimate modern metaphysics.

(italics ours) has supplied us in these cases with the terms 'instinct' and 'will.'"— "An analysis of the instinctive actions has yielded the result that the purposeful motions of animals frequently depend upon mechanisms which are a function of the symmetrical structure and the symmetrical distribution of irritability on the surface of the body of the organisms"— "The will actions of animals, *i. e.*, those motions which are executed consciously, will not be discussed here as I have already analyzed them in another book. I will simply state here that I consider consciousness the function of a definite machine or organism, which we may call the mechanism of associative memory. Whatever the nature of this machine may be, it has one essential feature in common with the phonograph, namely, that it reproduces impressions in the same chronological order as that in which they are received." But it is needless to quote further. Loeb has set up two straw-men, one 'metaphysics,' the other 'anthropomorphism,' and he is always attacking some statement under the guise of the one or the other.

On the other hand, he has raised up a fetish — that of a physico-chemical explanation of all life phenomena. He does not seem to understand that the psychologist will welcome with an enthusiasm equal to his own every advance he or any other biologist can make in the chemistry of living matter. The problems of the psychologist do not end when artificial protoplasm is made. On the contrary, were the biologist to succeed in fashioning it, the duty would be incumbent upon the psychologist to take this physico-chemically produced protoplasmic *X* from the biologist, and to test 'him' (pardon the anthropomorphism!) with respect to his color vision, temperature sense, etc., in short with respect to his whole sensory motor equipment. Likewise the psychologist would test him for the presence of the functions of memory, association, conception, etc. All these things and more would have to be done carefully before our experimentally produced abiogenetic individual[1] could be bottled in alcohol, shelved and labeled 'explained.'

In the February, 1907, number of the *Journal of Experimental Zoology* (pp. 151–156), Loeb claims that Jennings and others have misunderstood his theory of tropisms and have not quoted sufficiently from his earlier works. In this paper, Loeb claims that he never said that all reactions of lower organisms take place in accordance with

[1] Loeb says (p. 223), that experimental abiogenesis is the goal of biology. With the present rate of progress in physiological chemistry, a few years ought to see artificial protoplasm. Happy science that thus sees the end of its dreams so near! I fear, however, that some hardy, metaphysically inclined biologists fail to agree with Loeb.

the tropism schema. "Those who are familiar with the terminology of the physicist will most readily understand the difference between the two types of reaction if I state that heliotropism, *e. g.*, depends upon the value of i where i is the intensity of the light, while in Unterschiedsempfindlichkeit the reaction depends upon the value of di/dt, where t is the time."

Loeb further contends that he was first to point out "that there exist types of reactions which are as different from tropisms as are quantities of the dimensions of an acceleration from those of the dimensions of a velocity. My aim was to analyze the behavior of animals from a physico-chemical point of view and substitute the methods of modern science for the anthropomorphisms of the metaphysicians." We quote from this paper because it apparently represents Loeb's latest standpoint in regard to animal behavior. It likewise shows that he still fails to grasp the fundamental principle of psychology — viz., that a physico-chemical statement of behavior can never *interfere with* nor be *substituted for* a psychological statement.[1] He fails thus to understand the basal psychological fact which we try to instill into our 'first year' students of psychology.

J. B. W.

Modifiability of Behavior in Hydroides dianthus. V. ADA WATTERSON YERKES. Jr. of Comp. Neurol. and Psy., 1906, XVI., 441–450.

The stimulus used to produce the reactions which were studied in these animals by the above writer was a decrease in the intensity of the light. A black card-board screen was brought down rapidly between the window and the dish containing the worms. They react to this change in the intensity of the light by a quick withdrawal into their respective tubes. After from one to three such reactions to the shadow alone, the worms usually no longer respond to continuation of this stimulation. The animals show great individual variability in their reactions to this change in the intensity of the light. Some of the animals (10 out of 27) would not respond even to the first of the ten photic stimuli (the number usually given), while others responded each time to the ten successive stimulations.

Withdrawal into the tube was produced almost invariably by touching the brachial filaments with a glass rod. The reaction to a mechanical stimulus was so much more invariable than that to the

[1] *I. e.*, so long as we assume psychophysical parallelism as the working hypothesis of experimental psychology.

photic, that the investigator tried the effect of giving the photic stimulus and then immediately following this with the mechanical. When this was done, it was found that the worms responded more frequently to the photic stimulus alone than they did previous to their training, *i. e.*, they learn to react to the shadow.

J. B. W.

Death Feigning in Ranatra. S. J. Holmes. Jr. Com. Neurol. and Psychol., 1906, XVI., 200–216.

This article gives a study of two peculiar instinctive reactions in Ranatra. If a normal Ranatra is picked out of the water, it usually becomes motionless, either immediately, or after making a few slight spasmodic movements. This death feint may continue anywhere from a few moments to several hours. The state of death feigning may be made to continue almost indefinitely by stroking the body of the animal after it begins to come out of its feint. While in this condition, the body of the animal is in a state of tetanus and all sorts of unnatural positions, which the insect may happen to assume in the beginning of the feint, are retained for a long period of time. During this period of feigning, the animal shows no apparent signs of sensitivity. Holmes gives a table of the duration of successive death feints in several animals. The table shows quite clearly that the duration of the death feint diminishes with each successive trial.

It is also decreased when the insect is exposed to high temperatures, or to bright lights, and increased when subjected to low temperatures.

The removal of the supraœsophageal ganglia (by decapitation) causes a marked diminution of the time of the death feint. Holmes suggests that this is due to the heightened irritability, which follows when the inhibitory influence of this center is no longer exerted. When the body of the insect is cut in two across the middle of the protothorax, or across the hinder part of the thorax, the two parts of the body behave differently: The posterior portion comes out of the death feint more quickly than the anterior.

The writer also describes a second instinctive reaction which he calls 'deceptive quiet.' When a dish of water containing active Ranatras is approached, the animals cease their movements and lie for a time with outstretched legs. This state of deceptive quiet seems not to be related to the death feint discussed above. In the death feint there is muscular rigidity, while in the latter there is muscular relaxation.

This latter reaction is said by Holmes to be the characteristic attitude of Ranatra in the presence of its enemy. He says nothing of the teleological value of the death feint.

J. B. W.

The Reactions of Crayfish to Chemical Stimuli. JAMES CARLETON BELL. Jr. Comp. Neurol. and Psychol., 1906, XVI., 299–327.

By a very careful series of experiments, Bell found that the crayfish reacts to chemical stimulation on any part of its body, and hence we must assume that there are chemical sense organs all over the body, just as we know is the case in some fishes. The anterior appendages show the greatest sensitiveness for all stimuli, either because they are better supplied with sense-organs, or else more used in food-getting.

Many experiments were made to determine the various reactions of the animals to different stimuli. Meat juices are reacted to quickly with a positive chemotaxis. To lavendar-water, acids and salts, the reactions were rather indicative of a negative chemotaxis. The reactions to sugar and quinine were less definite. Quinine in general seems to exert a quieting effect.

When acid, salt, sugar and quinine were applied to the eyes, all caused retraction, acid and salt producing the most vigorous reactions. Hydrochloric acid caused reactions which gave 'every indication of pain.' Chemical stimulations with meat caused general restlessness and vague movements toward the source of the stimulation, but the animals seemed to depend upon contact sensations for the accurate localization of food.

The paper forms a good basis for further anatomical work on this form.

J. B. W.

Light Reactions in Lower Organisms. II., Volvox Globator. S. O. MAST. Jr. Comp. Neurol. and Psychol., 1907, XVII., 99–181.

This is one of the most completely worked out and best controlled pieces of research which has yet appeared, on the reactions of lower organisms to light. The writer's technique is good and his results are so accumulated that even the lay reader feels some confidence in the investigator's statements. The research, as regards its completeness, stands in marked contrast to a large number of studies on the reactions of lower organisms — studies which, for the most part, are dashed off for publication the moment a single point is gained.

Lack of space prevents a complete review of this paper. In it is

to be found a very thorough treatment of the anatomy of the eye-spots in *Volvox*, and the method of locomotion of this colonial form. The main part of the paper is taken up with orientation. It is found that the direction of motion in *Volvox* exposed to light is regulated by the relative intensity of the light on opposite sides of the colony, regardless of the direction of the rays. This, of course, will not harmonize with Loeb's oft-reiterated statements to the effect that the main feature in all phenomena of heliotropism is the fact that symmetrical points of the photosensitive surface of the animal must be struck by the *rays* of light at the same angle, etc., everywhere emphasizing the importance of the direction of the light.

Orientation in *Volvox* is not the result of trial and error reactions as is claimed by Jennings for *Stentor*, *Euglena* and other forms. *Volvox* colonies make no errors in the process of orientation. Likewise there is no evidence of a 'motor reaction' of the colony taken as a whole. Orientation is brought about, however, by motor reactions of the individuals which compose the colony.

J. B. W.

THE BEHAVIOR OF THE HIGHER INVERTEBRATES.

Du Rôle du Sens Musculaire dans l'Orientation de Quelques Especes de Fourmis. M. PIERON. Bull. Inst. Gen. Psych., Paris, 1905, IV., 168–187.

Pieron experimented with the ants *Aphænogaster barbara nigra*, *Formica cinerea* and *Lasius fuliginosus*, for the purpose of determining how they find their way home. He made the following experiments:

1. The finger was passed across the trail: The ants on each side of the line of disturbance halted and spread out along that line. After a while, some accidentally struck the trail on the opposite side of the line and resumed their march.

2. The path was moistened with an aqueous decoction of ants taken from an alien nest: The ants retreated precipitately.

3. The path was moistened with pure water: The movements of the ants were not affected.

4. The path was brushed crosswise with odoriferous herbs: The ants hesitated momentarily, then passed on.

5. The dust of the path was displaced by a twig: The ants were not disturbed.

6. A shallow groove was made across the path: The ants hesitated, felt about with their antennæ, turned back and after a time passed on in the original direction.

7. Obstacles were placed in the path of the ants: They behaved as in the sixth experiment.

8. A familiar object was removed from the path: The ants behaved as in the sixth experiment.

9. An object was placed in their path, and after the ants had become accustomed to it, it was placed in another portion of the path: The ants were not disturbed by changing the position of the object.

10. Their eyes and antennæ were painted with opaque pigments: The ants continued along the trail in the usual manner. The ants in this condition, however, were likely to attack the workers of the same colony.

11. A sheet of paper sprinkled with fine soil, bits of turf and other detritus was placed across the path of home-bound ants: Whenever one of the ants mounted this trap, the whole structure was transported to a new situation in a place topographically similar to the path along which the ants had been moving. In each case, the ant continued in the direction it was going, about as far as the distance between the trap, in its original position, and the nest.

The responses recorded above are those made by *Aphænogaster*. The responses of *Formica* and of *Lasius* varied somewhat from the above. Pieron, however, thinks they did not vary enough to invalidate the following conclusions.

1. Odors play a part in the life of the ant, but it is not by olfactory sensations alone that ants are guided on their journeys. In orientation on an individual or common road, the muscular sense plays a considerable, essential and probably unique rôle.

2. These experiments furnish evidence of a third mode of orientation, which consists, firstly, of a muscular memory of the various movements necessary to go from one point to another, and secondly, of a reversible memory, permitting a return to the original place independently of the faculty of orientation by vision or by the 'sens labyrinthique de l'espace,' or by following a trail by means of responses to olfactory stimuli. It seems justifiable to agree with Pieron that the kinæsthetic sense plays some rôle in the activities of ants; but these experiments certainly do not prove the existence in ants of a reversible muscular memory functioning in the manner contended for by Pieron. My own work gives experimental data which militate against this idea of Pieron.[1]

C. H. Turner.

The University of Chicago.

[1] See below the editor's review of Mr. Turner's preliminary paper.

The Habits of the Tent-Building Ant (Cremastogaster lineolata Say). WILLIAM MORTON WHEELER. Bull. Amer. Mus. of Nat. Hist., 1906, XXII., 1–18.

In this paper, Dr. Wheeler makes a critical study of *C. lineolata's* habit of sporadically constructing tents over colonies of aphids and coccids. These tents, which are occasionally constructed by other ants, are composed of agglutinated earth or vegetable detritus. In the words of Dr. Wheeler, "These structures are of local and sporadic occurrence, as if owing their origin to some unusual condition in the environment rather than to the normal instincts of the species." The material for study consisted of several carton tents obtained in the sandy barrens about Lakehurst, New Jersey. The paper contains an excellent historical resumé and numerous excellent illustrations. Indeed the paper contains the first adequate figures of these structures.

As to the possible function of the tents, Dr. Wheeler mentions four suppositions: (1) they prevent the escape of the aphids and coccids to other plants or other parts of the same plant; (2) they protect these insects from their natural enemies or other ants; (3) they protect the aphids and coccids from cold; (4) they protect the ants from exposure. To the best of my knowledge, Dr. Wheeler is the first scientist to lay stress on the fact that the protection from cold afforded the aphids and coccids by these tents would be of value to the ants; however, he thinks that the main use of these tents is to exclude more powerful competitors for the excreta of the insects. To use his words: "It is not improbable, therefore, that *C. lineolata* in constructing tents over its charges merely emphasizes its sense of proprietorship in the presence of the larger and more powerful ants with which it has to compete in the struggle for existence; and it may well be that the tents are constructed only in the localities where such competition is unusually severe."

Although the utilitarian value of the tents is manifest and to some they may seem admirable examples of foresight and reason, yet the author is convinced that their construction is due not to intelligence but "to hereditary, instinctive disposition in all colonies of the species, but manifesting itself only under conditions formerly prevalent or universal, but now of rare and sporadic occurrence." The inference that this sporadic tent-building is a reversion to an ancestral habit is supported by the following facts: (1) in the tropics, many genera live in suspended carton nests; (2) in the tropics, a few species of *Cremastogaster* live in carton nests; (3) in this latitude, *C.*

lineolata, in constructing its nests under stones, sometimes uses agglutinated detritus; (4) in at least one instance, *C. lineolata* has been known to construct a carton nest in which the whole colony resided.

C. H. TURNER.

THE UNIVERSITY OF CHICAGO.

On the Founding of Colonies by Queen Ants, with Special Reference to the Parasitic and Slave-Making Species. WILLIAM MORTON WHEELER. Bull. Amer. Mus. Nat. Hist., 1906, XXII., 33–105.

This paper, an acquaintance with which is indispensable to students of the parasitic and slave-making species of ants, is the result of a large series of experiments made by compelling artificially 'deälated' female ants to consort with small colonies of alien workers. Animal psychologists will be interested to note that some of the conclusions are opposed to certain of Miss Fielde's contentions and to learn that Dr. Wheeler has placed much emphasis upon the then little recognized fact that "the instincts of the ant species have their center of gravity, so to speak, in the female and not, as is usually supposed, in the worker."

In the paper, three methods of founding colonies are discussed; the typical, the redundant, the defective. Typically, the female, unaided, founds a colony and raises her first brood, feeding them by converting her fat-bodies and degenerating wing muscles into food. During this time, the female fasts. In the second case, the female not merely founds, unaided a colony and raises her first brood, but she carries over to that colony and cultivates certain plants which, for countless generations, have been raised by the stock for food. In the third case, the female cannot form a colony unless aided by the workers of some other species.

Under this third or defective method of forming a colony, Dr. Wheeler recognizes three conditions: temporary social parasitism, permanent social parasitism, and dulosis or slavery. Temporary social parasitism is where the female seeks and obtains adoption into a queenless colony and gets those workers to raise her young. When these young have matured, they emancipate themselves from their hosts. This emancipation is secured either through the death of the host species or the emigration of the guest species. Permanent social parasitism begins in the same manner as temporary social parasitism, but the guest species and the host species continue to dwell together as one colony. Dulosis begins in a different manner. A solitary

female enters a small colony of an alien species, and, after killing the workers, adopts and raises their progeny as the first step towards the bringing up of her own offspring. Later the offspring of this female make raids upon other colonies, using a portion of the booty for food and raising the rest as slaves.

Dr. Wheeler's observations seem to warrant the following conclusions: (1) the workers of one species are hostile to females of other species; (2) this hostility is not always manifested with the same intensity; (3) females are sometimes adopted by adult workers of the same species but of different nests; (4) animosities among ants are not in all cases reactions to unfamiliar odors; (5) slavery has no direct ontogenetic nor phylogenetic connection with temporary social parasitism; (6) probably young and vigorous females of nearly all species, when confronted with a few hostile ants and their brood, will kill the ants and take possession of the brood.

C. H. TURNER.

THE UNIVERSITY OF CHICAGO.

The Queen Ant as a Psychological Study. WILLIAM MORTON WHEELER. Popular Science Monthly, 1906, LXVIII., 291–299.

This is simply a recasting in a popular form of certain facts supporting Dr. Wheeler's contention that the female is psychologically the highest developed member of an ant colony.

C. H. TURNER.

THE UNIVERSITY OF CHICAGO.

A Preliminary Note on Ant Behavior. C. H. TURNER. Biolog. Bull., Vol. XII., 1906, 31–36.

There are several significant points in this paper. First, Turner shows that ants do not slavishly follow back the homeward odor trail as is maintained by Bethe and others.

This fact is proven by the author's rather ingenious method of having the ants learn to mount a card-board stage for pupæ. After many random movements, the ants learned the way from the stage to the nest and back. When the association was established, a second inclined plane was so placed as to lead from the opposite side of the stage to the nest; since no ants descended by this new route, the evidence is clear that all the ants had learned to go home by the other path. The first incline, which had become thoroughly scented by the passage of the ants, was now exchanged for a fresh, unscented one. The scented incline, however, was not discarded but was placed on the

opposite side of the nest, replacing the unscented one there. "Thus there was an unscented path in the position of the old trail and the old familiar scented path was in a new position."

The ants now, if they are 'reflex machines' "should either have spent approximately as much time learning the way down the new incline as they did before, or else, in their random movements they should have happened upon the scented incline and gone down it."

But the ants went almost immediately down the unscented path! These group experiments were controlled and the results there obtained were affirmed by using marked individual ants. By further experiments, Turner shows that the direction of the light plays an important rôle in determining the course of the ants.

Other interesting statements are made in the Note, but the details of the experiments leading to them are omitted until his complete paper appears.

J. B. W.

The Habits, Instincts and Mental Powers of Spiders, Genera Argiope and Epeira. JAMES P. PORTER. *Am. Jr. of Psychol.*, 1906, XVII., 306–357.

On the historical side of this paper, Dr. Porter has gleaned from various sources and put into summarized form the main psychological facts of interest concerning the behavior of spiders. This part of the work is so well and compactly done that one is almost immediately given the proper setting to the writer's own observations.

To better observe the reactions of the spiders, they were brought into the laboratory and supplied with as natural an environment as was possible. Such subjects as the choice of a place for a web, the nest and material for the nest, the manner of building the web and its variability are discussed. A quantitative measure of the variability of instinct was determined by counting the elements in parts of the webs.

Further instinctive activities were studied, such as the time of spinning the web, and the stimulus which releases the movements concerned in this act, feeding habits, web-shaking, mating, etc.

Some controlled experimental work was done towards testing the factor of adaptability in the reactions of these spiders, but the present work is merely 'eine bahnbrechende Untersuchung'—Dr. Porter promising a later experimental paper.

This type of study will have to be undertaken more and more often now, as comparative psychologists begin the study of new forms. The older studies by the general 'naturalist' are practically worthless as a point of departure for scientific work on behavior.

J. B. W.

Les Abeilles n'Exécutent — Elles-que des Movements Reflexes? GASTON BONNIER. L'Année Psychologique, 1905, 25–34.

This is a controversy between Bonnier and Netter as regards whether certain reactions in domestic bees are reflex or 'intelligent.' The reactions in question are as follows:

1. 'Le soleil d'artifice' or 'Parade' of the bees. This reaction can best be observed on bright sunny days before hives containing many young bees. "One perceives the numerous workers before the gum, flying in larger or smaller circles with head turned towards the entrance of the gum; the exercise once terminated, the bees all enter the hive." Netter holds that this is a mere 'reflex,' while Bonnier claims that the 'parade' is gone through with only by young bees who are habituating themselves to the entrance to the hive.[1]

2. The act of fanning or ventilating — referring to their habit of disposing themselves along the landing to the hive with head directed towards the entrance and body held stiffly erect, all the while rapidly moving their wings. Netter says this is due to need of respiration; Bonnier holds that the reaction is necessary to evaporate the water from the day's collection of honey. The number of animals reacting in this way is proportional to the amount of honey collected — "les ventileuses exécutent un travail réglé par la colonie et qui a pour effet de faciliter la sortie de vapeur d'eau, l'évaporation du miel jeune et so concentration, en établissant methodiquement dans la ruche un courant d'air, d'autant plus fort qu'il ya plus de récolte et par consequent plus d'eau à évaperer."

3. The act of cleansing the hive — the carrying away of dead bees, larvæ, etc. Netter explains this on the basis of the high irritability of the bee, saying that the workers on leaving the hive in the morning stumble over the debris and becoming 'furieuses' take the objects outside. Bonnier contends that this reaction is a striking example of the execution of work 'commandé et parfaitment réglé par la communanté.'

The fourth refers to the act of guarding. Certain bees on the outside of the entrance to the hive have apparently the function of guarding the hive from parasites, enemies, etc. Netter holds that this is a reflex function, the stimulus to attacking the intruder being its strange and too lively movements. Bonnier claims that the act is intelligent.

The whole contention is tedious. Better controlled observation and less discussion would benefit most of the research productions of the German, Swiss and French investigators referring to the behavior of ants and bees. J. B. W.

[1] In quoting from Netter, Bonnier does not state what constitutes the stimulus to the reflex and the original articles are not accessible to me at Tortugas.

THE BEHAVIOR OF BIRDS AND MAMMALS.

Further Study of the English Sparrow and Other Birds. JAMES P. PORTER. Am. Jr. Psychol., 1906, XVII., 248–271.

As its title indicates, the above paper reports the results of some further studies on the English sparrow. In addition to the half-dozen English sparrows under observation, a cowbird (♀), a dove-cote pigeon (♂), and a passenger pigeon (♀) were experimented upon at the same time.

The birds were tested with a simple maze and with a food-box. The maze was offered to the Vesper sparrow, the cowbird and the four English sparrows. The vesper sparrow made the poorest showing, whereas the cowbird and the English sparrow made records about equal. On the memory test for this maze, which was made after an interval of thirty days, all the birds made a good showing; the cowbird showed the best results, however, having 'forgotten' almost nothing of her experience in learning the maze a month previous. When the birds were tried in the reversed maze, it was found that the time of the first records of the birds was very high. The old habit of going in a certain direction persisted and only gradually gave way before the new conditions. These experiments on the maze go to show that "the birds do not depend upon sight alone for their cues as to where to turn and in which direction, but on a sense of direction and distance as well. That this is, at least in part, in terms of muscular sensations, is probable."

The food-box (latch inside, opened by pushing or pulling any one of four strings on the outside of the box) was tried upon the cowbird and the two pigeons. All of the birds learned to get the food, adopting, especially at first, different instinctive reactions for getting open the door. The birds are too few for any kind of inter-comparison as regards differences in intelligence to be made.

Some tests were made for the purpose of getting at the ability of these birds to discriminate 'designs' — glasses containing food but differently marked. Some experiments were likewise made to determine their ability to discriminate color. These latter tests are so unsatisfactory that the reviewer will neglect them.

The article as a whole is rather unsatisfactory because of (1) the small number of animals employed; (2) the wild state of most of the birds, especially of the English sparrows; (3) of the very limited number of tests made.

We venture to call in question the wisdom of averaging the first and second trials, the third and fourth, etc., instead of presenting the

separate time of the first, second, third, etc., trials. Such a method of presentation makes comparison with the work of others impossible. Likewise, the paper presents all *times* in seconds instead of in minutes and in decimals thereof. It is exceedingly difficult to get any immediate meaning out of 670 seconds, 810 seconds, etc. The decimal and metric systems were long ago adopted for all scientific purposes, and in the reviewer's opinion, it is wise to adhere to them in researches on animal behavior. J. B. W.

The Mental Life of the Domestic Pigeon. JOHN E. ROUSE. Harvard Psychological Studies. Vol. II., 1906, 581–613.

This paper reports an 'investigation of certain emotional and associative processes of the domestic pigeon.' The writer's work on the emotional processes appeared in the *Jour. Comp. Neurol. and Psychol.*, 1906, III., 161–162. This notice will, on that account, deal only with the associative processes of the pigeon discussed in the paper.

After a brief review of the literature of the subject, the writer, in characteristically ambiguous and involved phraseology states his purpose 'to determine the sense-data which the process involves, its method of formation (with due regard to social conditions), its rapidity, permanence, and modifiability, and also its probable degree of complexity. Material contributing to the subject was secured by observing the behavior of the animal when seeking to obtain food by overcoming such obstacles as labyrinths with wire passages, and latches, when the food was left in view or by finding it when out of sight." Tests upon the formation of sound associations were made, and also upon the possibility of learning by imitation. Considerable attention was paid to tests as to the influence of position, color and form, and the possibility of the formation of associations by employing such means.

In general, the investigator states that pigeons form associations only by the trial and error method, the learning curve being similar in contour to those reported of other animals. "Visual, acoustical, probably tactual, and certainly organic data, are the principal sensory factors of such associations." These are fairly permanent but easily modified.

"While these birds seem mentally inferior to English sparrows and to various mammals which have been tested in a similar manner, they are capable of numerous ready adjustments. They discover circuitous labyrinth passages, they learn to manipulate latch apparatus when adapted to their natural habits and conveniently placed, and they

easily reach their food by depending upon the position, color, or form of the box containing it." There is nowhere evidence of learning by imitation.

The paper is valuable as pioneer experimental work upon pigeons, but the reviewer feels that many of the tests were not well enough controlled to justify the conclusions drawn from their results.

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THE UNIVERSITY OF CHICAGO.

The Imitative Tendency of White Rats. CHARLES SCOTT BERRY. Jour. Comp. Neur. and Psychol., XVI., 1906, 333–361.

The purpose of this study is to determine to what extent rats learn by imitating one another.

Ten rats, four of which were black and white, the remainder albino, were used in the series of experiments with problem boxes requiring more or less manipulation. In each case, the solution of the problem consisted in obtaining freedom from the box in order to reach food outside. Seven such boxes were employed, though the conclusions seem to have been mainly drawn from a consideration of results with one box, which involved the pulling of a bent wire with the teeth and forepaws.

An untrained rat was put into the box and if he failed to get out within a certain length of time, a rat trained to the solution of the problem was also put in. The trained rat left the box at once, and the untrained rat, by 'seeing' the act performed 'learned' to accomplish the result. The writer states that the untutored rat 'watched' the other, and that 'when one followed another, it was with a knowledge of the end to be gained by that following.'

While the untrained rats undoubtedly learned to leave the box, the experimenter has not shown that the learning process was one of voluntary imitation. Furthermore, he has not shown the rat to be capable of such acuity of vision as he assumes. In view of the experiments of Small and of Watson which render it highly problematical whether the white rat uses vision for accurate orientation, definite proof of this point is indispensable. The contention for the presence of the function of voluntary imitation in these animals needs to be supported by a much more varied series of facts than is offered by Berry before it can be accepted by the majority of psychologists.

Owing to a lack of careful summarization and to too much condensation of the experimental material, such facts as are offered are inadequately put forth.

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Kinæsthetic and Organic Sensations: Their Rôle in the Reactions of the White Rat to the Maze. JOHN B. WATSON. Monograph Supplement to Psychological Review. Vol. VIII, No. 2.

The author, working with both normal and defective animals, confirms the conclusions of Small to the effect that the white rat learns and later traverses a labyrinth path largely by means of kinæsthetic and organic sensations. Visual data, if they exist at all in the animal, are certainly not used by it in associations like those formed in the maze.

These experiments should make it imperative upon every one experimenting with animals for the purpose of demonstrating the presence of imitation and other higher mental processes to show beyond a doubt that the animal in question can react definitely to a variety of simple and complex visual stimuli.

J. B. W.

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BOOKS RECEIVED FROM AUGUST 5, 1907 TO SEPTEMBER 5, 1907.

Proceedings of the Aristotelian Society, New Series, Vol. VII. Containing the Papers read before the Society during the Twenty-eighth Session, 1906–1907. London, Williams and Norgate, 1907. Pp. 244. 10 sh. 6 d. net.

Twenty-third Annual Report of the United States Civil Service Commission for the Year Ending June 30, 1906. Washington, Government Printing Office, 1907. Pp. vi + 202.

Behind the Scenes with the Mediums. DAVID P. ABBOTT. Chicago, Open Court Publishing Co., 1907. Pp. vi + 328. $1.50, net.

Hygiene of Nerves and Mind in Health and Disease. AUGUST FOREL, M.D. Trans. from Second German edition by HERBERT AUSTIN AIKENS, Ph.D. New York, G. P. Putnam's Sons, 1907. Pp. vi + 344. $2 net.

Sociological Papers. Vol. III. Published for the Sociological Society (London), New York and London, Macmillan, 1907. Pp. xi + 382. 10 sh. 6 d.

Ricerche di Psicologia. Volume Secondo. R. Istituto di Studi Superiori di Firenze. Florence, Tipografia Cooperativa, 1907. Pp. xxv + 148.